Referee Comments and Responses

In the revised draft all comments from the referees have been addressed. The revision is a substantially revised manuscript that involves a simpler model compared to the former draft and incorporates the referee comments and extensions from the comments. In the following, the comments from the referee are provided in numerical sequence with responses and changes in *italics*.

General Comments from Reviewer 1

1. The authors frequently mention and purport the superiority of the VAR model approach to the planning models which have been utilized by the Army Corps of Engineers in the past to estimate traffic levels on the river. Unfortunately, these claims are difficult to substantiate without any model statistics to evaluate or compare. These results should be offered and evaluated in the paper. The authors should instead present this study as an alternative modeling approach, with both strengths and weaknesses. And if they wish to claim that one approach is more applicable and appropriate than the other, a full comparison of the different approaches should be offered. Therefore, a fuller development of VAR models and how they work is needed.

We added a footnote referencing three papers written by Anderson, Sargent, and Stock and Watson where the strengths and weaknesses of VAR models compared to traditional structural models are discussed. Sargent and Stock and Watson are leaders in this area. We also attempted to highlight that the two approaches are complementary and hope we did this successfully.

2. Regarding the choice of utilizing the VAR model, only one short footnote is offered for choosing a one-period lag. The length of lag period isn't a trivial matter and may lead to model misspecification if not performed correctly. There are several criteria available for helping identify the most appropriate lag length, including both Akaike and Schwarz's criterion. This evaluation and discussion should be included in the analysis.

Lag lengths are determined using AIC tests, and other lag lengths were investigated (though this is not presented in the paper) as a robustness check. Table 2 and the discussion surrounding it have the lag length tests. The model is different than in the previous draft with the result that the lags are different too.

3. Another issue that should be addressed regarding the VAR model is that of contemporaneous correlation. One problem with these types of models is that global shocks to the region or system may result in some degree of correlation between the error terms. Events such as floods, lock repairs or policy changes that aren't part of the model are likely to influence many variables during the same time period. Some explanation should be offered as to how this is addressed in the study.

Various orderings were investigated and, as is evident from the results as well as the variance-covariance matrices we examined, there is very little evidence of contemporaneous causal effects in most directions. Thus, the particular ordering, which makes assumptions about contemporaneous causality, is not critical to the results. That is, the VCV/correlation matrix we examined indicated that common shocks are not a dominant feature of the data.

4. A brief discussion or summary is needed on how this analysis will inform the debate and what needs to be done with the demand models that are currently being used by the Corps.

We attempted to address this issue throughout the revised draft. The last paragraph of the conclusion is one prominent example. This was a helpful suggestion.

5. The literature review, especially as it relates to VAR models, is practically non-existent. Some discussion should be mentioned regarding where these models have been applied and utilized most (Federal Reserve) (Anderson, 1979) (Fomby and Hirschberg, 1989).

These papers, and a few others, are now in the paper. This will help to place the paper in context and reflects a needed addition.

6. A full graphic map of the region is needed and critical in the report. Otherwise the reader, unless knowledgeable of lock numbers, rivers and locations, can't evaluate the reasonableness of the findings.

We have added figure 1 to the paper.

7. The variance decompositions presented are quite useful and seem to resemble theory and reality. They show how dynamic and lagged the adjustments in the market place really are.

We agree, and with the substantial revisions, we continue to believe that the VDCs are very important.

8. Throughout the paper results are presented in their naïve form, without any discussion of the theoretical or common sense of the findings. This may be the result of using the VAR; it certainly minimizes the usefulness of the findings in the report.

This too was a helpful suggestion and we have added structural model interpretations to the description of the results.

9. The Corps may need to go back to the structural equation models that reflect real work conditions, using this VAR as useful indicators of relationships but not relying on them totally. A workshop of (three to five) agricultural transportation experts,

examining one by one the findings of this analysis would be very helpful. Some of the current findings make only modest economic/real world/empirical sense.

We agree.

10. The guts of the findings simply support Baumel and this reviewer's assertions in the two NRC's reviews that a spatial equilibrium analysis is needed rather than the partial equilibrium models that have been used.

We hope the comments we added at the end of the paper reinforce this. Further, it is noted that in <u>www.corpsnets.us</u> there is a substantial body of research related to spatial equilibrium modeling currently on-going by ACE.

11. The title should show what part of the inland waterway system is being examined in this analysis. It isn't the full system.

The title has been changed in accordance with this suggestion.

Specific Comments from Reviewer 1

1. Section 1, Page 1, Paragraph 1. A full treatment of the existing literature would be a welcome addition. The authors shouldn't assume the reader is as knowledgeable regarding VAR models as the authors.

We have added discussion and footnotes in response to this request. For example, besides the new discussion in the text, we added a footnote referencing three papers by Anderson, Sargent, and Stock and Watson where the strengths and weaknesses of VAR models compared to traditional structural models are discussed.

2. Section 1, Page 2, Paragraph 1. The quasi-trashing of the structural model approach (currently used by the Corps) isn't necessary nor documented. It either needs to be supported or eliminated.

If we appeared to trash the previous work while looking into potential strengths and weakness of each approach, it was not intended. We have revised this discussion dramatically to rectify this point. The papers cited support the questions raised about some aspects of the structural approach, but this discussion is relatively more balanced. Indeed, if the analyst is sure of the structural model, data are available and, the model is tractable with data, then structural modeling is preferred.

3. Section 1, Page 2, Paragraph 2. Again, unsubstantiated assertions about the weakness of the structural econometric models and their assumptions are presented.

See responses to 1 and 2 wherein this and related comments are addressed.

4. Section 1, Page 3, Paragraph 2, Line 1. Incomplete sentence. "In the next section, a nineteen variable VAR model."

Sentence no longer in paper since it is now a six variable model and this was edited out in the rewrite.

5. Section 1, Page 2, Paragraph 2. Again, unsubstantiated assertions about the weakness of the structural econometric models and their assumptions.

See response to 1 and 2.

6. Section 2, Page 5. Footnote 6: What was utilized to determine those variables most or least likely to be affected contemporaneously?

Theory, an examination of the results, and an examination of the contemporaneous variance-covariance matrix are all included in the revised draft.

7. Section 2, Page 6, Paragraph 1. Generally, a definition of "Shock" should be given. Can the reader assume the "shock" is always positive and do the results change if it is negative?

Yes, the reader can assume that and it is so noted in the paper. The definition and connections to underlying structures have been added throughout the paper as well.

8. Section 3, Page 7, Paragraph All. The authors should be careful in stating that one variable "causes" a response in another variable without some discussion of causality. There may be an estimated relationship between the two variables, but it may be a stretch to argue one caused the other.

In the first instance of the use of the term causality, it is changed in the revision to the familiar "Granger causality" so that it will be clear precisely what is meant by that statement.

9. Section 3B, Page 8, Paragraph 3, Line 2. Please define "noisy" for the reader.

This sentence is no longer included in the paper.

10. Section 3C, Page 10, Paragraph 1, Line 11. What is the importance of having no intermediate period where the effects diminish, since there is no consideration of the empirical implications of the findings?

The results and write-up have changed and this is no longer present in the revised version.

11. Section 3F, Page 15, Paragraph 1, Line 8. Why is this case "noteworthy"? Is it empirically or mathematically noteworthy? Is it consistent with logical or theoretical expectations?

The results and write-up have changed and this is no longer present in the revised version.

12. Section 3F, Page 16, Paragraph 1. These implications do not make sense without further explanation.

The results and write-up have changed and this is no longer present in the revised version.

13. Section 4A, Page 16, Paragraph 1, Line 1. Minor editorial suggestion in first sentence. Variance decompositions decompose......to Variance decompositions separate.......

This has been changed as suggested.

14. Section 4A, Page 18, Paragraph 3, Line 2. Typo. "Recall there is only one case, a shocks to a downriver barge rate....

The results and write-up have changed and this is no longer present in the revised version.

15. Section 4F, Page 21, Paragraph 1, Line 10. There must be a typo here.....the numbers don't sum correctly.

This table has been replaced, and the related discussion has been checked.

16. Section 5, Page 21, Paragraph 2, Line 1. Again, the statements of causality are presented when no proof of the direction of causality has been shown. If the authors simply mean "the response to a shock is", then those words should be utilized. Throughout the report, the variables identified as "causing something" flies against some existing empirical and theoretical knowledge of the industry. So, use the statement as a mathematical relationship if that is what is intended, and identify it as such early in the report.

As noted in 8, in the first instance of the use of the term causality, it is changed to the familiar "Granger causality" so that it will be clear precisely what is meant by that statement.

General Comments from Reviewer 2

My specific comments to improve this study are as follows:

1. There are hardly any references provided in the "Introduction" section to back up their statements. Authors are making so many vague statements and I believe some of the statements are not correct.

a) Page 1, lines 3 and 4, "Generally, these relationships are identified....has been heavily criticized." The authors should provide some references to back their statement.

We have revised the text and added a footnote referencing three papers by Anderson, Sargent, and Stock and Watson where the strengths and weaknesses of VAR models compared to traditional structural models are discussed to improve the paper in accordance with this suggestion.

b) The impression I get by reading the first two pages of the introduction is that the authors are claiming that structural models are static. The structural models could be static or dynamic.

The wording has been changed to "static or dynamic" models to make this clear. Thanks.

c) To support Footnote 1, on Page 1, the authors need to provide some references.

Done, see above in a).

d) The authors need to provide some references to support the top 6-7 lines on Page 2.

Done, see above in a).

2. I would like to see the authors provide plots of the raw data and the descriptive statistics. What are the units of measurement of these variables?

Plots are provided. The data are logged so the differences in the data are percentage changes.

3. I suggest that authors use the Dickey-Fuller, Augmented Dickey-Fuller and Phillips-Perron tests to test for unit roots. The authors should use at least 5-6 tests and should address the issue of whether the variables are stationary or not.

Done.

4. The authors have not justified why the log is needed.

This is standard practice for variables that grow over time.

5. The key question to answer is whether the variables are co-integrated or not. If the variables are co-integrated, then the VAR model is not the right model to use.

CI tests are performed and presented in the paper. In the new model, we did not find evidence of co-integration.

6. The ordering of the variables need to be justified because all the impulse response and variance decomposition results are sensitive to the ordering of the variables. I suggest that the authors check the robustness of the results with a few different orderings and also report this. The authors should also use some statistical criteria e.g. final prediction error (FPE) to obtain the ordering of the variables.

Various orderings were investigated and, as is evident from the results as well as variance-covariance matrices we examined, contemporaneously there is very little evidence of causal effects in most directions. Therefore, the particular ordering, which makes assumptions about contemporaneous causality, is not critical to the results.

7. In this kind of study, if VAR is the correct specification, then there is no reason to specify the same lag length for each variable. The authors should select their lag lengths based on some statistical criteria. I suggest FPE criteria that will also help in specifying the ordering of the variables and the lag lengths.

Unbalanced VARs are not standard practice and are difficult to use, e.g. OLS equation by equation is no longer the most efficient estimation method, complicating the analysis substantially. In the revised, we have maintained balance.

8. The authors need to report all the parameter estimates and the corresponding asymptotic t-statistics. They need to discuss briefly the significance and interpretation of coefficients.

Because these are reduced form estimates and because there is typically multicollinearity among lagged variables, it is not standard practice to present these. VDCs and IRFs were developed to overcome these problems. We have kept the standard practice, avoiding misuse of the parameter estimates.

9. The authors need to address the issue of whether one unit shock is more appropriate for their analysis than one standard deviation shock and why.

This is simply a scaling issue in the diagrams. Either will suffice, but the standard practice is to use the standard deviation shock.

10. Overall, the 19 variable model is too large unless it is absolutely needed. Either they should reduce the size of the model, or a couple of smaller models of "key" variables need to be estimated to investigate the robustness of the innovation accounting analysis.

The size of the model has been reduced considerably. There is now one representative variable from each of the six categories of variables, and the variables are generally expressed as relative rather than absolute prices.

11. On Page 5, the presentation of the model needs an improvement. I suggest that they write their model in vector and matrix notations to make it clear to the reader. My understanding is that they are estimating 19 equations, is that so?

This is a standard VAR model and so there are indeed 19 equations. However, in the revised draft the number of variables has been reduced. The presentation has been significantly altered. Therefore, vector notation seems unnecessary, particularly with the smaller model.

12. Since there are so many findings, I suggest that the findings should be put in a matrix form (or a table). Then if needed a second matrix (or a table) of key findings should be provided.

Again, the dimension has been reduced considerably and all results are in the paper or the appendix.

13. In the discussion of results, Sections 3 and 4, the authors have just interpreted the graphs or tables. The important issue, which is missing is the explanation of results. For example, on page 10, first paragraph, the authors need to address why these results are so, i.e. the authors need to explain their findings. They have just reported without explanation. In these two sections can the authors address—wherever possible—the explanation of their findings- what could be the reasons of happening such and such etc.

This has been added to the paper in a number of different ways as suggested.

14. In the variance decomposition section, the authors have combined the VDC numbers as reported in the tables. First, they need to report VDC for all 19 variables. I believe it is important to know which rail rate and which barge rate is contributing to VDC. After a detailed discussion of the results (all 19 variables) they should provide some summary results where they can combine the VDC components of variables in each category.

In the revised draft, all statistics are reported.